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Discussion of
"ARTIFICIAL PRECIPITATION CONTROL"

by Ray K. Linsley
(Proc. Paper 547)

RAY K. LINSLEY,* M. ASCE.—Mr. Howell is much concerned that the paper under discussion is out-dated. It is true that nearly two years have elapsed since the paper was originally prepared. However, there seems to be little if any change in the situation during this time. With the advantage of over a year, Mr. Howell is hard-pressed to find any reports by independent agencies demonstrating significant success in a cloud seeding venture. In fact he cites only two, one of which is listed by the writer (Reynolds, Table 1) which showed very slight increases in precipitation from one of several storm types analyzed. The unpublished paper by Barton to which Mr. Howell refers has not come to the writer's attention. However, Beaumont, Work, Frost, and Barton(1) find significantly less precipitation during one seeding year on the Rogue River, Ore., and Beaumont(2) finds no evidence of precipitation increases after evaluating several projects in Washington and Oregon.

The four references cited by Mr. Howell as favorable evaluations from organizations which have contracted for cloud seeding are unpublished and the writer cannot comment on these. Mr. Howell cites several references (7, 8, 9, 10, 11, 12) by cloud seeding firms and states that the failure of independent agencies to contradict these evaluations is evidence of success. As a matter of fact, almost every evaluation by an independent agency has contradicted reports of seeding firms. The conduct of an adequate evaluation is a time-consuming operation and it is not surprising that some seeding projects have not been evaluated by outside groups. This is particularly true of the three foreign projects included in Mr. Howell's references.

Mr. Howell also takes issue with some of the factual statements in the original paper. This is no doubt partly the result of the variety of differing experimental results reported by various investigators. However, with respect to the effective temperature for nucleation, Schaefer(3) shows the threshold temperature for a number of natural ice nuclei to vary from about +10° F to -30° F. The temperatures at which these same materials become completely active vary from about -13° F to -40° F. In another paper Schaefer(4) reports determinations of the ice nuclei in the air at Mt. Washington, N. H. A detailed summary of the observations is lacking, but the data presented shows that nuclei counts between 0.5 and 10 nuclei per cubic centimeter were observed about 10 per cent of the time and as much as 40 per cent of the time during some winter months.

Mr. Howell implies a degree of exactness in our understanding of precipitation theory which hardly seems justified in the light of conflicting opinions expressed in the literature and the rapid changes in thought which have been taking place during recent years. The "elegant" raindrop growth computations to which he refers are mathematical computations based on simplifying

* Associate Prof. of Hydr. Eng., Stanford Univ., Stanford, Calif.

assumptions and hypotheses. Such computations combined with very skillful observational work are leading toward a much better understanding of the precipitation process, but categorical assertions with respect to this process now seem a bit premature. The writer must plead ignorance with respect to Mr. Howell's reference to the "Phlogiston theory of heat." The dictionary defines phlogiston as "in old chemistry, the principle of inflammability, a hypothetical substance formerly thought to exist in all combustible bodies." The relation between this definition and anything in the original paper is not obvious.

In contrast to his desire for explicit statements with respect to the theory of precipitation formation, Mr. Howell sweeps aside some of the practical problems rather lightly. His analogy between alloys and cloud seeding seems a bit overdrawn. It is true that useful alloys were developed long before the metallurgical theory which explains their properties. However, an alloy made by trial can be subjected to rigid physical tests and its properties and utility definitely determined. Rainmaking on the other hand is not so easily proven. Such statements as "Practical experience indicates that the trigger effect of seeding is not unimportant" and "The fact of overriding importance relative to artificial precipitation control is still the striking effect that silver iodide smoke is observed to have on the structure and development of suitable clouds and the precipitation from them" are generalizations which certainly are not yet conclusively demonstrated. Much of the argument advanced by cloud seeders is based on the a priori assumption that seeding does increase rainfall and that therefore any anomalies in the rainfall regime of a seeded area are the result of seeding.

By undisclosed statistical methods, Mr. Howell concludes that the probability of chance departures being as favorable as those of the writer's Fig. 3 is 0.075. Actually, the data of Fig. 3 represent the most favorable of three target areas in Santa Clara County, Calif. Lieberman's analysis of all the data indicates the probability of chance occurrence of the observed departures to be about 0.25. Mr. Howell's argument that a number of individually insignificant results may in the aggregate be statistically significant, ignores also the persistence of precipitation patterns which often results in large differences between an annual precipitation pattern and the normal. His argument might also be used to suggest that intuitively, a large number of negative evaluations suggests a lack of success more firmly than any single evaluation.

The writer must admit to having misquoted the results of the Pend'Oreille Basin project. Actually seeding was conducted for only 10 days instead of three months as implied in Table 1. Since Mr. Howell quoted one paragraph from this report it seems best to repeat the entire conclusions which do not leave the same impression as the single paragraph. The full conclusions[5] are:

"It is the consensus of the Committee, on the basis of the analyses made, that it cannot reach a definite conclusion that the Bonneville Power Administration cloud seeding operation was effective in increasing runoff in the Pend Oreille River Basin.

Comparisons of runoff showed consistently higher amounts from the target area with respect to the control areas for the months of August, September, and October 1951, September being the most abnormal of the three months.

While the cloud seeding activities conducted under the contract were definitely associated in respect to time with the high September runoff within the Pend Oreille River Basin, it must be recognized that this runoff might have been due entirely to natural conditions.

Since the Bonneville Power Administration cloud seeding operations lasted only ten days and were complicated by other seeding activities conducted in the region before, during, and after that period, no clear-cut evaluation was possible by available techniques. Any increases in precipitation from seeding which might have extended into the control area would have had an effect in reducing the contrast between the control and target areas and thus obscure influences which may have occurred on the target area.

The analyses indicated that, even if the conditions of the seeding operations had been ideal, one period of seeding would not be sufficient to demonstrate conclusively its effectiveness toward increasing precipitation and runoff.

The Committee believes that there is need for more comprehensive studies of cloud seeding operations. Coordinated efforts should be directed toward the development of evaluations based on more thorough knowledge of physical and chemical processes and improved analytical methods."

The paragraph quoted by Mr. Howell represents a hypothetical computation of possible benefits from the project based on estimates of the cloud seeding firm made before any seeding was undertaken. As may be seen from the conclusions of the evaluating committee, there is no evidence that any such benefits were realized.

Mr. Howell, in discussing the hydrologic problems associated with rainfall augmentation, states "In practice some part of the storage capacity (of a reservoir) is used many times over in the course of a water year." In regions of marked seasonal variation in rainfall or in arid regions where runoff occurs only sporadically, there may be no chance to refill storage during any water year. The example cited by the writer is simplified from conditions which exist in California, where inflows during the winter and spring months are stored and released for irrigation and other uses during the summer months. A substantial quantity of water is wasted in many years because reservoir storage capacity is inadequate to hold it all. Inasmuch as it is not possible to tell in advance which years will produce inadequate water supplies, cloud seeding projects would presumably be initiated every year and the seeding firm would expect full payment whether or not natural rainfall subsequently proved to be adequate.

While in general one can agree that the incremental runoff from added rainfall will be greater than the average rate of runoff, the percent of runoff from large storms rarely, if ever, approaches 100 per cent and from small storms may be nearly zero. Increased runoff during large storms may be detrimental because of flooding, and seeders are often instructed to refrain from seeding major storms. If seeding is limited to the small storms where the per cent of runoff is small, the gain from a small percentage increase in rainfall would be very small, indeed.

Throughout his discussion Mr. Howell stresses cloud seeding operations for industry and points out that these are supported as experimental research. Continued research in the field of cloud seeding is certainly justified but the support of such research does not imply that industry is confident of the outcome. Many research projects of the past have failed to yield fruitful results. Moreover, income tax laws and utility rate regulation can make such research quite inexpensive to the sponsoring industry. Mr. Howell says little of the many seeding projects which have been supported by civic or farm groups which do not have any technical support within their organizations. Seeding firms often, by inference at least, suggest that large increases in precipitation

(50 to 100 per cent) are possible. In many cases the sales presentation pictures cloud seeding, not as an experiment subject, but as a proven technique. That many such projects have been terminated by sponsoring organizations is evidence of the doubt which develops in the mind of even non-technical personnel.

Mr. Howell is correct that several of the writer's conclusions are similar to those of the Council of the American Meteorological Society (reference 8 of Bibliography). The reference was unfortunately transposed from its proper position during preparation of the manuscript. It is quite probable that the council will issue a revised statement in the future as they have done on one occasion in the past. This does not imply, however, that the revision will endorse cloud seeding.

Mr. Howell is particularly concerned with the conclusion that seeding offers no relief from serious and prolonged droughts. This seems to be one of the most obvious of the conclusions, since if there are no clouds there can be no cloud seeding. The endless sunny days which often characterize a major drought provide little encouragement for increasing rainfall.

It is doubtful that the writer's arguments will have changed Mr. Howell's position any more than the writer's opinion has been changed by Mr. Howell's discussion. Certainly, if Mr. Howell is to continue in the cloud seeding business he must believe in it. The writer certainly does not wish to have research in the field of cloud seeding terminated, but rather, hopes that future experiments will be planned and conducted in such a manner that effective statistical evaluation is possible. Certainly the conclusions must be based on observed facts and not on faith or intuition. As of now, the writer still adheres to his conclusion that there has not yet been any definite proof of the economic success of rainfall augmentation.

On page 12 of the original paper Huff and Stout are quoted as finding that "six per cent or less of the atmospheric moisture was precipitated during a storm." Their studies were actually based on monthly periods and their conclusion would be more explicitly stated by saying that "less than six per cent of the moisture passing over Illinois during any month was precipitated."

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Discussion of
"CORROSION AND CORROSION RESEARCH"

by Thomas R. Camp
(Proc. Paper 685)

EDWARD W. MOORE.[1]—This paper, like the author's two previous papers on the subject, is a comprehensive, highly condensed presentation of the electrochemical theory of corrosion, as it applies to conditions encountered in water supplies. With minor exceptions, the material presented is already in existence in the more advanced textbooks of electrochemistry, and in the literature of corrosion. The author's contribution is to assemble the material in such a way that it can be assimilated and applied by engineers sufficiently interested to devote the study necessary to follow the author's condensed rigorous mode of presentation.

To one interested in corrosion theory for itself, and in research that will lead to better understanding of the nature of the corrosion process, the worth of the paper is obvious. It may be less obvious to the engineer interested primarily in using corrosion theory to evaluate protective coatings and corrective water treatment processes. The author states that the principles he presents have been useful to him for these purposes. This aspect of the matter is not, and probably should not be, emphasized in the present paper. However, it may be hoped that a paper or series of papers emphasizing the application of the theory to particular instances will be a "next" in the author's plans. It is suggested that such papers may convince a larger number of engineers that the rigorous study called for by this and the preceeding papers will pay off.

The author is quite correct in deploring the small amount of attention given in past and current research to the development of corrosion theory, in contradistinction to the vast effort expended on evaluation tests. It is not improbable that a little more attention devoted to basic theory would have eliminated the need for many an expensive testing program.

No important exception can be taken to the theory as presented in the main body of the paper. The facts, and the pertinent equations,—chemical and thermodynamic,—are set forth with clarity and precision. The selection from the large mass of data available has been judiciously made to include those elements most significant to water works practice. A careful reading and re-reading of the paper will provide a sound knowledge of the electrochemical theory of corrosion and its potential applications.

Certain minor criticisms might be offered. One could wish, for example, that the significance and potential utility of the equations developed in the section on Polarization Tests had been more completely discussed. It will certainly not be clear to most engineers where this material leads.

In the second paragraph of page 685-16 it appears to be implied that chemical reactions always give slurry precipitates, and only electrochemical reactions give wall coatings. While it is a well-known fact that rapid precipitations

1. Associate Prof. of Sanitary Chemistry, Div. of Eng. and Applied Physics, Harvard Univ., Cambridge, Mass.

in the presence of large amounts of chemical reagents give slurries, many slow precipitation reactions result in coatings on solid surfaces. Supersaturated solutions of many calcium and magnesium salts produce suspended precipitates only with difficulty, but readily deposit on the glass walls of their containers; crystallization from solutions often starts at the walls; boiler scales not only form at walls but adhere tightly to them. Then there is the whole range of adsorption phenomena to be considered.

This comment does not, however, imply rejection of the author's conclusion that calcium carbonate precipitation on the walls is associated with the electrochemical reactions. Some recent research at Harvard, using radioactive calcium to form the calcium carbonate, gives strong support to the author's thesis, in that the amount of calcium carbonate precipitated is directly related to the amount of iron corroded for the first few days of immersion of the specimen.

The writer of this discussion has a semantic objection to the term "plate out," when applied to the electrochemical decomposition of materials like hydrogen ion, bicarbonate ion, normal carbonate ion, and the like. The term is derived from the electro-plating of metals, which is a special application of electrochemical decomposition resulting in a true surface plating. It has no useful function when applied to non-metallic materials, and merely confuses the issue.

Reference should probably be made to the latest and most complete tabulation of chemical thermodynamic values, from which, as shown by the author, oxidation potentials may be derived. (As a matter of fact, much thermodynamic data is now derived from carefully measured oxidation potentials.) This is "Selected Values of Chemical Thermodynamic Properties," circular number 500 of the National Bureau of Standards, published in February 1952. It will also be noted in this publication that the thermochemical calorie is now defined as 4.1840 absolute joules, and that Faraday's constant is 96,485.3 coulombs. However, it will be a long time before such minor revisions in values are of any practical significance in our field.

Discussion of
"PHOTOSYNTHESIS IN SEWAGE TREATMENT"

by William J. Oswald and Harold B. Gotaas
(Proc. Paper 686)

ISADORE NUSBAUM,[1] A.M. ASCE.—Much interest has developed in the past decade in the use of oxidation ponds for the treatment of liquid wastes from small and moderately sized communities, institutions, military establishments and industry. With increased construction and operating costs of conventionally designed sewage treatment works, oxidation ponds have been presented as a means to attain the ideal of a high degree of treatment at low cost. The use of oxidation ponds for the treatment of sewage has now become extensive, particularly throughout the southwest and far west regions of the United States, and yet little but empirical data is available for their design. The authors have presented basic data on the role of photosynthesis in sewage treatment. Much yet remains to be clarified before the fundamental data available through the study of phytoplankton can be used for the realistic design of a sewage treatment plant.

Until this occurs deviations from the present empirical data frequently used, organic loadings of 50 pounds of BOD per acre of surface area per day, minimum depths of three to four feet and minimum detention periods of 30 days, must be viewed with caution. This should not discourage any community from perhaps including such flexibility in the pond arrangement that these characteristics might be varied in the interest of obtaining pertinent data for the area and waste involved upon which to base extensions and operating practice.

The authors have divided oxidation ponds into two more or less artificial classifications: Type I ponds are those which by the authors' definition obtain the oxygen necessary for waste stabilization through surface reaeration and have detention periods of at least three to six weeks. Type II ponds are those with detention periods of less than one week and which are highly dependent on photosynthesis for oxygen. The qualification is then introduced that Type I ponds may obtain large amounts of oxygen through photosynthesis when conditions are favorable. A brief analysis of the situation demonstrates that photosynthesis or causes other than surface reaeration must be responsible for the stabilization which takes place in the many hundreds of oxidation ponds now in use in the southwest and which are characterized by the authors as Type I. The rate at which oxygen is transferred from the atmosphere to a water media through the air-water interface is dependent on several factors but primarily upon the extent of the dissolved oxygen deficit in the water. Where the water is saturated or super-saturated with oxygen there is no net transfer of oxygen from the atmosphere and indeed there may be a loss of oxygen from the water to air. The upper 12 inches or more of conventionally designed ponds are frequently super-saturated with oxygen and only for short periods, principally

1. Senior Water Pollution Control Engr., San Diego Regional Water Pollution Control Board, San Diego, Calif.

during hours of darkness, show oxygen concentrations appreciably below saturation. A typical example of such variation is pictured by Smallhorst, Walton and Myers.(1) The amount of oxygen which may be transferred through the air-water interface is limited by the characteristics of the pond itself. As an approximation, for example, Imhoff and Fair(2) state that for ponds about three feet in depth the rate of reaeration from the atmosphere at 100% oxygen deficit is 13 pounds of oxygen per acre of surface area per day. This condition rarely exists and, for the more realistic mean limiting condition of a 20% oxygen deficit, in ponds designed for loadings of 50 pounds BOD per acre of surface area per day the atmosphere would supply 3 pounds of oxygen per acre per day. It is quite evident that a deficit of 47 pounds of oxygen per acre per day would exist and also an extremely offensive anaerobic condition unless oxygen for aerobic microbial action were supplied by other means. Experiences in Southern California and Texas have shown that at the 50 pound BOD loading oxygen concentrations in the ponds are extremely favorable throughout the year.

Although pond loadings are conveniently calculated in terms of available surface area, depth plays an important part in the satisfactory operation of an oxidation pond. It is necessary to maintain at least three feet of water in the pond to avoid the growth of nuisance vegetation. Smallhorst et al(1) have pointed out that temperatures may become too high in very shallow ponds during the summer to permit the growth of algae such as Chlorella. Depth through the larger volume plant also permits the retention of greater amounts of oxygen during periods of super-saturation. Although the bottom layers may be anaerobic in deeper ponds this factor may contribute to the over all reduction of BOD which takes place. Otherwise all the carbon dioxide produced by aerobic bacteria from the organic matter in the waste might be used by the algae in the resynthesis of organic matter. Increased depths also permit the rapid dispersion of the incoming waste avoiding shock loading effects, which will be discussed later, and minimize the resuspension of solids from the bottom of the pond.

Recirculation has been found to have a distinctly salutary effect upon the operation of both ponds and the entire sewage treatment plant. Offensive odors surrounding some treatment works have been entirely controlled by the recirculation of pond effluent. Where the sewage is given some secondary treatment before discharge to the pond, the secondary units have been found to function with fewer problems when recirculation is practiced. The most important effect of recirculation is to reduce the shock loading of high BOD wastes on the pond.

The writer has observed with great interest the operation of many oxidation ponds in Southern California and doubts that the shallow, short detention period ponds proposed by the authors will withstand the intermittent high loadings that occur unless some form of recirculation is practiced or the ponds are merely used as a final treatment following secondary treatment.

The amount of recirculation is to be used has never been calculated by other than trial and error methods. Smallhorst et al(1) have recommended that provisions for recirculation of effluent be included in plants utilizing oxidation ponds and that recirculation ratios of 15 to 50% should prove satisfactory. The writer believes, based on experience, that recirculation ratios of at least 1 part pond effluent to 2 parts of sewage flow should be provided and will be necessary for recirculation particularly where primary effluent is discharged to the ponds. It appears likely that the amount of or necessity for recirculation is dependent upon the initial oxygen requirement of the waste.

Aerobic bacteria are necessary inhabitants of oxidation ponds since it is by their action that organic matter in the wastes is destroyed or converted to carbon dioxide to be used by the phytoplankton. Algae have been cultivated in synthetic media without the presence of bacteria and extraneous organic substances. Since coliform organisms and presumably pathogens have shown marked reductions in numbers in passage through oxidation ponds, it has been postulated that some antibiotic or toxic substance may be generated by the algae. However, as indicated by the authors no such change other than the normal die-away expected on storage has been observed in their experiments. The writer has found significant coliform densities in ponds with detention periods of 10 to 15 days. Effluent from ponds with 30 or more days detention are found to have bacterial reductions comparable to or greater than chlorinated sewage treatment plant effluents. Since the use of chlorination in small sewage treatment works is usually quite ineffective due to insufficient operation and maintenance, oxidation ponds have been used to produce bacterially safe effluents. For short detention period ponds it will be necessary to provide some means of chemical disinfection until more information is available on any bacterial hazards. Yet virtually nothing is known about the disinfection of pond effluents.

Several preliminary tests have been run by the writer on the chlorine requirements of oxidation pond effluents. The effluents have had low chlorine requirements in the neighborhood of 2 to 3 ppm. The coliform population was rapidly reduced to extremely low values on chlorination in this range. The effect on the algae was undetermined but they may have been destroyed also. If so, chlorination may decrease some of the pond effectiveness.

The authors have stated that there is little tendency for blue-green algae to grow in oxidation ponds. Reports of the presence of large numbers of blue-green algae have been reported from Texas. In the San Diego area two separate ponds, thirty miles apart, have been afflicted with an almost pure culture of a blue-green algae of the genus Arthrospira. The vegetative cells of this organism are found in free-floating filaments helical in shape. Apparently, organisms coming to the surface collect in small clumps which then are driven by the wind into pockets where large mats of the Arthrospira accumulate. As the surface of the mats putrefy an extremely foul, offensive odor is produced.

The effluent from these ponds was well treated nevertheless. Some investigational work has indicated that the growth of this specific algae is connected with the salinity of the ponds involved. The Arthrospira have previous ly been found in brackish water ponds. Both of the infested ponds have sodium chloride concentrations of about 3500 ppm, in one case due to saline ground water infiltration and in the other a slough with no outlet is being used as the pond.

As reported in an earlier paper[3] the total inorganic solids content of the sewage used for these experiments at the Sanitary Engineering Research Laboratory of the University of California is quite low. Data is lacking on the use of ponds in areas where the normal solids content of the water is high. It is true that synthetic media for the culture of algae are high in mineral solids, however such media have been carefully prepared to produce optimum growth.

There is some evidence[4] to indicate that relatively small changes in chloride concentration may have definite effects upon the cultivation of certain phytoplankton. Oceanside, California has experienced difficulties with an experimental oxidation pond having a surface area of 5 acres. The oxidation pond contains 800 to 1,000 ppm of sodium chloride due to the high salinity of

the community water supply. Many other factors may be involved and some have been investigated with no positive results. Attempts to seed Chlorella into one of the brackish ponds mentioned earlier were completely unsuccessful.

Krauss[5] has pointed out that much work remains to be done on the total nutrient requirements of algae grown in mass culture and that even in complex organic media with large numbers of compounds in solution it is possible that growth may be limited by the absence or unavailability of some element. Iron, zinc, copper, molybdenum, manganese, cobalt, and other elements are believed to be essential micronutrients for the cultivation of algae. It is quite conceivable that despite the heterogeneous makeup of sewage in some regions, the sewage may be deficient in some element necessary for a vigorous health growth of algae.

Little data has been found on the effects of insecticide formulations on the operation of oxidation ponds. Ponds do, under some circumstances, harbor mosquito and gnat larvae and health departments justifiably demand that insect control be exercised. In at least two cases with which the writer is familiar the spraying of ponds with diesel oil solutions of DDT were followed by almost complete algal inactivity. Much less impairment was suffered when a water type emulsion of lindane was used.

Despite the lack of knowledge about many aspects of the use of oxidation ponds for sewage treatment, they do serve a useful purpose in sanitary engineering. The limited information has been reflected perhaps in low efficiencies as compared to what might be possible.

The use of oxidation ponds for sewage treatment must be justified principally on their effectiveness and cost for that specific purpose. The recovery of valuable by-products, other than water for agricultural or industrial purposes, is in an elementary stage of development and does not merit consideration at this time in the design of oxidation ponds for sewage treatment.

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Discussion of
"SEDIMENTATION IN RECTANGULAR BASINS"

by Claes N. H. Fischerstrom
(Proc. Paper 687)

THOMAS R. CAMP,[1] M. ASCE.—The author's paper is a welcome addition to the literature on sedimentation and the design of settling basins. The paper is particularly valuable in that it presents performance data on full-scale settling tanks in confirmation of the principles of design indicated by rational theory. Such data are difficult to obtain because comparative performance data for poorly designed and well designed settling tanks are not available for most plants.

The author has obtained the comparative data by inserting longitudinal baffles and horizontal trays in existing poorly designed settling tanks, thus improving their design. The author's studies demonstrate conclusively that tray settling basins have great advantages in improving performance in the removal of flocculent settling matter over the performance obtained in deep settling tanks. The author's studies also show that long, narrow settling chambers are much to be preferred to wide, short chambers for the settling of flocculent suspended matter. These advantages have long been recognized in the settling of discrete particles. It is now shown in full scale tanks, as anticipated from the rational theory, that the advantages also hold for flocculent suspended matter.

Allen Hazen showed in 1904 that the proper criterion for the design of settling basins for settling discrete particles was floor area rather than tank volume; that is, overflow rate rather than detention period (Trans. ASCE, December 1904). The writer has demonstrated on a theoretical basis that the same criterion applies in the design of settling tanks for flocculent suspensions (Trans. ASCE, 1946). One of the reasons it has been so difficult to confirm these findings in actual plants is because of the overpowering influence of residual turbulence from the inlets. The author has recognized this difficulty and has taken steps to minimize the effect of inlet turbulence by means of long, shallow and narrow settling channels.

The writer has shown that an inherent characteristic of sedimentation basins is their instability of flow pattern, and that stability may be increased by increasing the Froude number. The Froude number may be increased by increasing the tank velocity or by decreasing the hydraulic radius of the cross section, or both. The author in adopting this idea proposes that Froude's numbers in excess of 10^{-5} should be used for the design of settling channels for soft coagulated water.

The writer's studies indicate that tank velocities up to about 12 fpm may be used as a means of obtaining stability and high Froude numbers, the limiting velocity being determined by the critical velocity for scour of settled sludge. In contrast with this procedure, the author prefers to establish the maximum velocity by means of an upper limit for Reynolds' number. The

1. Camp, Dresser & McKee, Boston, Mass.

author selects a maximum value of Reynolds' number of about 500 on the thesis that higher values will cause turbulent flow in the settling basin which will interfere with sedimentation. The cost of such a procedure by the author is to limit the tank velocity to much lower values than the writer believes are necessary.

The author refers to a paper by Langelier who describes a tunnel section of a water supply line which was successfully used for alum coagulation and settling of a turbid and colored water, despite the fact that the velocity in the tunnel was about 12 fpm. It is evident in this case that a 12-ft velocity was inadequate to scour the sludge, else it would not have settled out. In interpreting these results, one must keep in mind the great length of tunnel sections, 8,700 ft, in which to dissipate the residual turbulence of the inlet. The value of the Reynolds' number in this case was about 26,000 in terms of the hydraulic radius.

The turbulence in a settling tank is of two kinds, that induced by the drag on the walls and floor and the residual turbulence from the dissipation of the kinetic energy at the inlet. It is the writer's opinion that it is next to impossible to induce stream-line flow in a settling tank because of the residual energy from the inlets. In a recent discussion by the writer (Proceedings Separate No. 689) of a paper on settling tanks (Proceedings Separate No. 590) it was pointed out that the ratio of the loss in energy at the inlets to the loss in energy caused by drag on the walls and floor was 11,600 to 1 for one experimental tank and 2,800 to 1 for another experimental tank. The overpowering influence of inlet turbulence should be obvious from these figures.

The author emphasizes the importance of uniform distribution of the influent among basins and uniformly over the cross-sectional area of each basin. This distribution requires relatively high inlet losses with the kinetic energy dissipated as turbulence in the tanks. The designer cannot have both uniform distribution and low kinetic energy at the inlet of the tank if the distribution is made directly to a battery of settling tanks. If, however, settling tanks are made sufficiently long and are preceded by flocculation chambers at the upstream end, the problem of inlet design is removed since the velocity from the flocculation chamber to the settling tank may be made very low.

The tray settling tanks designed by the author for use as secondary settling tanks in the activated sludge process of sewage treatment are unique. The writer believes that they should give satisfactory performance, and he hopes that performance data from the actual plant will be available in the near future.

Discussion of
"BIOLOGICAL TREATMENT OF HIGHLY ALKALINE TEXTILE MILL WASTE-SEWAGE MIXTURE: PROGRESS REPORT OF THE INDUSTRIAL WASTE SECTION OF THE SANITARY ENGINEERING RESEARCH COMMITTEE OF THE SANITARY ENGINEERING DIVISION"

(Proc. Paper 750)

A. W. PETTY [1] A.M. ASCE.—The writer has read with interest Paper No. 750, ASCE Proceedings on Biological Treatment of Highly Alkaline Textile Mill Waste—Sewage Mixture. The "accepted tradition among waste treatment engineers not to consider biological treatment of a waste with a pH over 9.0 unless with a preliminary neutralization" is certainly not an accepted tradition at our sewage treatment plant where we have been treating sewage with a pH between 9.0 to 10.0 with excellent results on a 120' diameter trickling filter for 10 years. In fact we have to raise the pH to that point in order to get results. (Our present flow is around 1.75 mgd.)

Our plant consists of pre-chlorination, screening, grit removal, primary sedimentation, chemical precipitation, filtration, final sedimentation, and post chlorination. Originally it was planned to use chemical treatment during the summer (the tourist season) and the filter in winter. However, this not only required starting a new filter growth every fall but the filter alone gave insufficient treatment considering the point of effluent discharge, and chemicals alone were very expensive and failed to give sufficient B.O.D. removal. Therefore, both are used year around.

Use of copperas and use of alum both proved unsatisfactory and in experimenting with pH adjustment with lime it was discovered that lime alone not only gave best results but that at the proper alkalinity the filter growth changed from black to green, B.O.D. reduction in excess of 95% was obtained, odors vanished and a sparkling clear effluent was produced. Basically, we convert the bicarbonate alkalinity of the raw sewage to carbonate with chemical hydrate. However, complete conversion is unnecessary in winter and during low flows in cold weather presence of hydroxide tends to kill the filter growth. On the other hand during high flows and hot summer weather it is necessary to enter the hydroxide range and the filter growth will tolerate as much as 50 p.p.m. hydroxide.

The pH of the raw sewage varies from 6.7 to 7.3 usually between 6.7 and 6.9. The filter influent pH varies from 9.6 in winter to above 10.0 in summer (Highest 11.2). Through the filter the pH drops to around 7.3 to 7.5 and the pH of the final effluent is around 7.0 to 7.1.

Where normal sewage plant lab tests (B.O.D., settleable and suspended solids, etc.) tell what has happened, alkalinity tests of the filter influent tell what will happen using this type of treatment.

The writer thought the above information might be of some interest in connection with the problem discussed in Paper No. 750. It seems to us unnecessary to spend time to determine if sewage of pH 9.0 to 10.0 can be treated on

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1. Director of Public Works, City of Virginia Beach, Va.

a trickling filter when here it is an established process, in fact the most efficient method of treatment.

NOTE: Since writing the above the writer has discussed this problem with Mr. R. H. Souther from whose research data Paper Number 750 was prepared. Mr. Souther points out that his research is concerned more with the volume of alkali than with pH. We have found that while filter growth cannot be controlled by pH values it can be accurately controlled with alkalinity tests. Tests at this time show 200 ppm carbonates, 30 ppm hydroxide, and a pH of 10.1. An excellent filter growth is being maintained.

Discussion of
"INDUSTRIAL WASTE PRACTICE -
SOME THOUGHTS FOR SANITARY ENGINEERS"

by John E. Kinney and John C. Bumstead
(Proc. Paper 779)

EDMUND B. BESSELIEVRE,* M. ASCE.—Industrial wastes pollute streams and interfere with sewage treatment processes. Prevention of stream pollution and design and operation of sewage treatment plants are the province of the sanitary engineer. Therefore, he is the logical one to handle industrial waste problems.

Solution of industrial waste problems requires a knowledge of sanitary engineering principles. It also requires contact—conference—argument with State, municipal, regional and Federal health protection agencies. Sanitary engineers are in constant touch with these authorities and are familiar with their objectives.

It is true that solution of many industrial waste problems requires knowledge of chemical processes, plant operations, etc. but the proper solution of any waste problem, whether it may be reached by normal sanitary engineering methods or means or requires additional technique, requires team play. The team is the consultant—the industrial plant officials—the regulating authority concerned. Unless these three elements work together with mutual trust and confidence the result may be a sad one. Thus, if the waste in question is of a complex nature beyond the normal scope of training for a sanitary engineer the plant using the process will have chemists or chemical engineers available to assist the consultants with their specific knowledge.

The consultant should not hesitate to utilize the plant personnel in fields outside of his normal jurisdiction. He cannot be expected to be omnipotent in all fields. There is no disgrace in admitting that some particular phase of a complex chemical process is out of his line. The plant personnel will be complimented by being included in the study.

Also, as industrial waste projects are usually an expense item to an industrialist the consultant can save money for his client by definitely using plant personnel to obtain needed data. They know the ropes and can go freely throughout the plant, getting samples, collecting data, where an outside engineer might be prevented from getting a complete story, due to security regulations or the reluctance of plant operators or officials to give data to a stranger.

This paper brought out the place of the equipment manufacturer in this field. The great advances in sanitary engineering in general—water, sewage and industrial waste treatment—are due in the main to the policy of mechanizing operations, developing special units to achieve certain results, the origination of new methods and processes to utilize these new means.

As stated, the manufacturers are organized and able to spend the funds

* Cons. Engr., Manager, Industrial Wastes Div., Kaighin & Hughes, Engineers-Constructors, Toledo, Ohio.

necessary to develop new and economical units and ideas. But no one manufacturer has all the elements, all the methods, processes or systems to enable him to provide the panacea for all types of wastes in all cases. In many cases waste problems can be solved without the use of any equipment or process.

The sanitary engineer should not depend upon the manufacturers to solve the waste problems upon which he is called to advise. He should make it his business to be acquainted with all the available units of all manufactueres, all methods, processes and systems of treatment. In fact, after careful study, he may develop an entirely new combination of units of several manufacturers to provide a proper, practical solution for a given case. This is not asking too much of a sanitary engineer who has kept abreast of his field.

After he has studied the case, evaluated and weighed all the very many factors in any industrial waste case and has then decided upon the proper plant, if any is needed, to carry it out and equipment and processes of a certain make and type are indicated then he should discuss the problem with the manufacturers or proponents of that equipment or process. Every manufacturer knows his own equipment best, its advantages, its limitations, its scope and he should be granted the privilege of discussing the use of any of his products, or his methods, to assure that they are being properly applied. Any manufacturer, in fact, should insist on having the facts in any problem before being asked to make a recommendation or a quotation. Many plants have been failures because there was not this entente between the designer and the manufacturer and the engineer used only the catalog data as a basis of design. The manufacturer should be expected to provide the engineer with all proper dimensional data to design the structures necessary to his unit but he should not be expected or permitted to design the plant. The engineer is receiving a fee for that and he should maintain his independence by asking the manufacturer only for the help rightfully to be expected.

The three elements in the proper solution of any industrial waste problem are complete confidence and cooperation by the plant owners with the consultant, complete freedom of the consultant to obtain essential facts and complete frankness on the part of public officials concerned so as to be reasonable in their demands and understanding of all the elements—physical, economical-practical in any case.

PROCEEDINGS PAPERS

The technical papers published in the past year are identified by number below. Technical-division sponsorship is indicated by an abbreviation at the end of each Paper Number, the symbols referring to: Air Transport (AT), City Planning (CP), Construction (CO), Engineering Mechanics (EM), Highway (HW), Hydraulics (HY), Irrigation and Drainage (IR), Power (PO), Sanitary Engineering (SA), Soil Mechanics and Foundations (SM), Structural (ST), Surveying and Mapping (SU), and Waterways (WW) divisions. Papers sponsored by the Board of Direction are identified by the symbols (BD). For titles and order coupons, refer to the appropriate issue of "Civil Engineering" or write for a cumulative price list.

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c. Discussion of several papers, grouped by Divisions.

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